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Going back to re-study communities: challenges and opportunities

Graham Crow

Professor of Sociology and Methodology, School of Social and Political Science, Chrystal Macmillan Building, 15A George Square, University of Edinburgh EH8 9LD email g.p.crow@soton.ac.uk

Going back to re-study communities: challenges and opportunities

Abstract: Community re-studies can inform our understanding of what members of communities want from research and the extent to which they feel that they get it. The history of community research suggests that what is expected of researchers by community members can differ from what it is felt that they actually deliver, especially if the published reports have a critical edge. Challenges relating to this legacy of previous studies can be encountered by researchers returning to fieldwork locations. Re-studies also provide opportunities to rectify matters, but these may be beyond the control of researchers who go back to communities studied previously.

Keywords: community; re-studies; expectations; research impact

Introduction

This article addresses the issue of what can be learned about how research on communities is received through projects in which research is undertaken in communities that have been researched previously. It does so by first considering what researchers in the field of re-studies have reported in terms of their rationales for re-visiting communities and the problems that they have encountered in relation to the reception of previous work. These problems are outlined and then five sources of such problems are identified and discussed: researchers responding to academic criticisms of romanticism; researcher misrepresentation; media misrepresentation; researcher overpromising; and mutual misunderstanding. The article then concludes by considering various strategies that are available to reduce these problems so that the benefits that community research (including re-studies of communities) has the potential to provide may continue to be available.

Problems in the field

Undertaking community re-studies has a number of rationales, the most important of which is that they have the potential to shed light on the social change that has taken place in that community in the intervening period. As Jan Breman has put it, 'Returning to a location that has earlier been subjected research offers an excellent opportunity to check the pace, direction and effects of social change' (1997: 53). In addition, restudies allow the findings of the original study and its underlying assumptions to be re-examined (Crow2012; Burawoy 2003; Kloos 1997; Seale 1999: ch.10). In general, re-studies are undertaken at least a decade after the original research was conducted, allowing long-term processes of social change such as modernization, urbanization and changing gender relations that are the subject of macro-level theorising to be explored in a community context. In some cases the gap between the original study and the re-study amounts to several decades, and in such circumstances the likelihood is greater that differences in findings will be attributable at least in part to changes in the research perspectives adopted. Thus Göran Djurfeldt and his colleagues returning to India a quarter of a century on from their original fieldwork which had been conducted at the end of the 1960s reflected that they had moved away from their earlier thinking that had been framed in terms of dependency theory: 'Twenty five years is more than half the working lifetime of a researcher, and it is not only the village and the villagers that have changed, but so have we' (1997: 177) In undertaking such research, many things have come to light about the reception of the original researcher and his or her (or, in the case of research teams, their) research outputs. This reception may be positive, as in the case of Djurfeldt and his colleagues who refer to villagers who spoke to them as 'our friends' (1997: 182), but it can also be hostile. In cases where re-

studies are undertaken by the original researcher, the reception that they receive can be intensely personal.

Returning to the site of her investigations in the west of Ireland a quarter of a century on from her original fieldwork, Nancy Scheper-Hughes (2000: 324) was not welcomed by all community members and was subjected to 'drumming out of the village' by some. Several locals had not taken kindly to her study, *Saints, Scholars and Schizophrenics*, published first in 1977, for its sympathetic yet critical analysis of community decline framed in terms of her understanding of how social relations in small, tight-knit communities can be oppressive to individuals by virtue of the scrutiny to which community members are subject. Her return was prompted in part by a sense that her approach may have led her to 'an overly critical view of village life in the mid-1970s' (2000: 320), but she found that time had not yet been 'a great healer' (2000: 328). Her book had clearly touched a nerve.

Half a century earlier, Art Gallaher returned to 'Plainville', Missouri, which had been studied by James West fifteen years previously (West 1945). West told Gallaher that 'to the best of his knowledge, with one outstanding exception most Plainvillers had taken his report "with relative composure"'. But Gallaher had people saying to him, 'I certainly hope you are not here to do the same thing that feller West did a few years ago... Folks here are mighty unhappy with him... Some would like to lynch him'. Their accounts of their dissatisfaction were expressed in the language of betrayal of trust. Even residents who were more favourably disposed were unsure. For one, West was 'A nice guy, but he asked a hell of a lot of questions' (Gallaher 1971: 286, 288-9, emphasis in original). Members of the community had hosted West so that he could conduct his research, and had implicitly expected and trusted him to do something other than he actually did, which in their view was to concentrate in his book on the negative aspects of the community. With this in mind, Gallaher included in his book 'A Note to Plainvillers' expressing his hope that they would appreciate that 'no analysis of community life can be completely pleasing and still be honest' (1961: xiv).

In 'Springdale', New York State, no one dared attempt a re-study after publication of Arthur Vidich and Joseph Bensman's *Small Town in a Mass Society* in 1958 led to the authors being hanged in effigy by the townspeople and cast as 'manure spreaders'. The authors 'had expected that Springdale "rightly" would be scandalised by our analysis', which (amongst other things) failed to achieve anonymisation. They had knowingly engaged in providing reassurances in pursuit of good 'community relations' between the project and the townspeople, promising 'a positive approach' from which they consciously deviated in writing the book in which they highlighted the town's parochialism. Vidich declined an invitation to go back, even though this was many years later, saying he 'lacked the inclination and emotional stamina' (2000: xxxii, 448, 491). The project has become a textbook case of how relations between communities and researchers, and also research organizations, can go awry (Bell and Newby 1971: 116ff). It serves as an extreme case, about which it is useful to know so that the difficulties encountered might be anticipated and avoided by researchers preparing to enter the field of community research.

Sources of fieldwork problems: researchers responding to academic criticisms of romanticism

A first reason why reports on communities may not meet with favour by members of those communities is that researchers have been at pains to avoid suggestions of romanticism, a criticism that has some foundations. Geoff Payne once asked why monographs in the community studies tradition are typically so full of 'nice' people. Having reviewed the accumulated literature, he was led

to the conclusion that 'the main impression generated is one of a world populated with pleasant, likeable people'. This ran counter to his experience: 'In the course of fieldwork in several locations in the past half a dozen years, I have encountered people whom I did not like, and situations that felt most unpleasant.... The people in community studies are too "nice"'. The explanation for what have come to be considered as rather romanticised accounts of community in which accounts of social life are too good to be true relates to problems of selective sampling and of 'selective reporting' (1996: 21, 22, 23). Through these processes, less likeable people and unhappier experiences may be screened out from research, leaving a sanitised account of local social relationships.

The criticism of community research presenting romanticised accounts has been levelled at the work of the Institute of Community Studies by Jocelyn Cornwell, whose selection of East London as the location of her research led her to reflect on how previous analyses of community there had contributed to 'the post-war myth of Bethnal Green as the model of urban village life'. Her distinction between public and private accounts that people provide of their lives allows the argument to be made that 'public accounts are selective and partial', and that a more rounded portrayal of community life requires penetration beyond this public face in order to access 'those parts of people's experience and opinions that might be considered unacceptable and not respectable' (1984: 24, 205). In her own research, the ideas of her participants about blame for ill-health that were contained in their private accounts are indeed uncomfortable when compared to the less judgemental views expressed in their public accounts, but the book is all the better as a work of social science for the inclusion of both. Community researchers are committed to doing more than descriptively reproducing public accounts built around mythical conceptions of harmonious communities.

The susceptibility of Michael Young and Peter Willmott's work on Bethnal Green to romanticism relates to a methodological problem of which they were well aware. In writing *Family and Kinship in East London*, they noted that although they had supplemented their interviews with ethnographic observations, it remained true 'that for the most part we can only report what people say they do, which is not necessarily the same as what they actually do' (1962: 14). They were as researchers cut from a different cloth to James West, whose conduct in Plainville earned him a reputation as an aggressive interviewer; as one community member put it, 'he didn't care if you didn't want to answer questions, he just kept asking them anyway' (Gallaher 1971: 294). By contrast, Young and Willmott were concerned not 'to create discord between our informants' (1962: 26), and as a result dropped potentially contentious questions about earnings. Their general approach has been taken to task for its lack of rigour, a criticism connected to their 'desire to popularise' (Platt 1971: 137). Mindful of such criticisms, social scientists have sought to go beyond the public face that community members present to outsiders. Ken Dempsey's study of 'Smalltown' in Australia is a good example of this. He shows that the notion of community members being like 'one big happy family' with which he was presented was at odds with the realities of the stigmatization and social exclusion of marginal groups in the community. Nonetheless, he found ways of sustaining a productive working relationship with members of the community over the best part of two decades while developing such analyses. The fact that 'Smalltownites are pragmatists who are sceptical about the value of all academic activities' (1990: ch.4, ix) will doubtless have helped in this process of accommodation. Other re-studies also indicate that people studied may exhibit a degree of indifference to what researchers say about them, preoccupied as they are with 'getting on with their lives' (Bryson and Winter 1999: 6). In addition, some members of communities that have been studied will be

persuaded by the core argument that many of them make about diversity within communities, including diversity of perceptions. As Jan Breman and his co-editors express it, 'The village is not the same village for all its inhabitants' (1997: 5), echoing in an Asian context the point made about British research by Ray Pahl regarding how one group's 'village in the mind' (1968: 271) can be quite at odds with that of another group, even though they both inhabit the same place.

Sources of fieldwork problems: researcher misrepresentation

Community researchers are, then, under pressure from their academic peers to question stories of local social life presented to them by community members because such narratives have a tendency to embody romanticised elements that are at odds with reality. The sociologist Norbert Elias would have had his own published work on community in mind when he described the role of the sociologist as 'a destroyer of myths' (1978: ch.2), because that research revolved around the (in Elias's view questionable) attitude among some community members that they were '*better* than the others'. Elias saw it as his role to interpret such thinking as the stuff of 'fantasies' (Elias and Scotson 1994: xv, xl111, emphasis in original), based as it is on the development of stereotypes in which key characteristics are exaggerated to implausible lengths. From this point of view, social scientists coming to a community from outside may be better placed to develop an analysis of relationships within that community than that based on community members' conceptions of themselves.

Elias and Scotson do not record how well their interpretation of local social relationships was received by members of the community that they studied, although they report that many participants in the study 'took a cheerful and encouraging interest in the research' (1994: xiii), without which the project would not have been possible. A similar recognition of the debt owed by researchers to members of communities being studied can be found in the acknowledgements of Norman Dennis and his colleagues' classic study of a Yorkshire coal-mining village *Coal Is Our Life*. These are worth quoting quite extensively because their tone is at odds with the views expressed to Dennis Warwick and Gary Littlejohn by townspeople when they returned to undertake a re-study some three decades later. The original research team had written that their 'greatest debt without question is to the people of Ashton. They have neither resented nor rejected our prying into their affairs. Whenever there was an opportunity for co-operation this has been extended to us. Although they remain anonymous, this is their book' (Dennis et al. 1969: 6). This account runs counter to the sense of betrayal by the authors of the original study that Warwick and Littlejohn had expressed to them. Several inhabitants told Warwick and Littlejohn that 'outsiders continually get the place and its people all wrong', which raises the question of how community research might accommodate local people's views. Put bluntly, 'Can a community study ever be a negotiated product in which sociologists are not the only definers of the situation?' (1992: 33). This issue will be returned to in the conclusion.

An important lesson of the study and re-study of Featherstone (the real name of the place given the pseudonym 'Ashton') is that memories of previous research can endure, especially if local people feel that trust has been betrayed by researchers who use the opportunity to portray their community in a negative light, although others with more positive evaluations of *Coal Is Our Life* were among those of the re-study interviewees who remembered it. Among the critics, some linked their objections to the arguments developed in the original study with comments on what they

regarded as poor research practice that had been at the root of the community being misrepresented. Thus one of Warwick and Littlejohn's participants 'also thought it bad because it gave a thoroughly distorted picture of the town. Some aspects of life were over-emphasised, even caricatured, some hardly alluded to or even ignored completely. He considered it a bad piece of research. The researchers, in his view, had come with firmly fixed stereotypes in their heads and with preconceived ideas. They had looked only for evidence which would support these stereotypes and preconceived ideas, and of course had not failed to find them'. For another interviewee the problem lay in over-reliance on older miners in pubs as sources, one of whom was 'able to keep up an endless stream of anecdotes and information, so long as the beer flowed'. The over-reliance of the original research team on talking to men has also been identified as a source of bias and misrepresentation, and Warwick and Littlejohn's decision 'to give priority to women as respondents' (1992: 32, 73) in their re-study was a conscious response to this.

Sources of fieldwork problems: media misrepresentation

One of the reasons why so many Featherstone residents were able to recall the original study after three decades may be the extensive coverage that the book received in the Yorkshire Post on its publication. This media coverage had portrayed the study as one that had 'an emphasis on the seamy side of life' (Warwick and Littlejohn 1992: 32), but to the authors this misunderstood their purpose of seeking to expose 'the cultural poverty and isolation imposed on the working-class community' by material deprivation. Reflecting on the book's reception more than a decade after its publication, Fernando Henriques rejected the view that he and his co-authors had been driven by 'a somewhat morbid preoccupation with the miners' past', arguing that the realism of their analysis had been vindicated by the failure of more optimistic scenarios that accompanied the nationalization of the coal industry and the development of the modern welfare state around the middle of the 20th century. That said, it is understandable how reproducing the view of outsiders of Ashton as 'that dirty hole' (Dennis et al. 1969: 8, 12) would be met by mixed emotions among the people whose village is so described.

Another example of the role of the mass media in influencing the reception of community research is provided by Margaret Stacey's first (1960) study of Banbury, which had its discussion of social status represented in the press as a description of 'a place pulsating with snobbery and riddled with class distinction'. This made it a challenge to meet the requirement of the re-study's funders 'that there should be evidence that the study would be welcome in the town', and the problem was compounded when an open meeting to publicise the re-study project was covered in the Oxford Mail under the headline 'New Probe Into "Snob Town"' (Bell 1977: 58, 57, 58). Colin Bell implies that such coverage was unfair because although the original study was 'not an affectionate book', it was also not one that involved 'disparagement of the place'. Bell's memories of working with Stacey on the re-study were that 'She loved being there. She really liked being in the town and liked the work, liked the people' (2008: 114); she had no intention of belittling them. A key lesson of this case is that academic discussions of social scientific concepts like status consciousness do not translate unproblematically into journalistic coverage of research.

We should be wary, however, of blaming the mass media entirely for instances of poor reception of research publications by the people about whom they have been written. It has been said that once a book is published 'its authors lose control over how it is to be understood, misunderstood,

interpreted, and misinterpreted', a comment made by Vidich and Bensman whose book's hostile reception has been mentioned above. But the genuineness of their claimed 'bemusement' must be questionable, since in the first edition of the book they noted how the local newspaper 'always emphasises the positive side of life; it never reports local arrests, shotgun weddings, mortgage foreclosures, lawsuits, bitter exchanges in public meetings, suicides, or any other unpleasant happenings'. It reproduces Springdale's 'image of itself' as 'Just Plain Folks', and epitomises the outlook in which 'A challenge to the image of Springdale as a preferred place cuts deep' (2000: xxv, 31, 29, 31). Thus just as Dempsey courted disfavour by writing about outsider groups in Smalltown such as the 'blockies' (people living in poor-quality accommodation on the margins of the community) and the 'no-hoppers' (1990: 45, 43) because their presence ran counter to the ideology of Smalltownites comprising one big happy family, so too did Vidich and Bensman for their explicit discussion of 'the shack people'. By drawing attention to this deviant group about whom 'In the course of ordinary activities.... there appears to be a tacit agreement not to recognise or mention their existence' (2000: 69, 70), they must have known the risk of becoming unpopular that they were taking by breaching this arrangement.

Sources of fieldwork problems: researcher overpromising

The hostile reception of community members to the publication of Vidich and Bensman's book has another dimension to it relating to the authors promising more than they delivered. Expectations of the sort of report on Springdale that they would produce were shaped by their responses during the period of fieldwork to questions about their purpose. When they were (reasonably enough) 'pressed by the community to tell what the study was about, who it included, what its purpose was, and what kind of book would be written' the project team developed what they called 'a line' that included various reassuring statements: 'We are not interested in the negative features of the town... A positive approach is needed... We are interested in constructive activities because from this we feel we can help other people in other communities to live better lives. Springdale is a laboratory which may help us find important solutions... We have to get back to the older values of the individual, neighboring and the neighborhood, and Springdale seems to provide an opportune setting for this. We enlist your cooperation in helping us to solve this scientific problem'. Vidich and Bensman go on to say that 'these commitments were made as a way of selling the project to the townspeople at a time when no one knew what the project would be studying', but to characterise the townspeople as overly 'sensitive' (2000: 447, 431) is to pay too little heed to the research team's responsibility for the ultimately disappointed expectations that they generated through overpromising regarding what their research would be like.

A particularly important aspect of overpromising by researchers relates to assurances about participants remaining anonymous. In the Springdale case, the research team responded to concerns about being identified as the sources of quotations through the reassurance that they were 'doing only a statistical report', and when the book turned out to include (in the words of Vidich and Bensman's critics) 'clearly recognisable Springdale individuals' (2000: 448, 449), the defence on which they fell back, the use of pseudonyms (for place and individuals) as standard practice in community research, predictably failed to appease. The promise of anonymisation may be hard to deliver even if greater care is taken over it than Vidich and Bensman did. According to Gallaher, 'A well-thumbed copy of *Plainville, U.S.A.* in the local library has the real names carefully pencilled by the pseudonyms invented by West' (1971: 292). An almost identical story is told about the first

Banbury study by Bell, although he adds that in this case the pseudonyms that 'have names of real people written against them' have had this done 'in many cases wrongly' (2008: 114). Even so, the point remains that anonymity among fellow members of a community being studied is difficult to achieve.

Overpromising can also occur in relation to what is said, or at least implied, about the benefits of research for members of communities being studied. Oscar Lewis called meetings as part of his re-study of the Mexican village of Tepoztlán and reported how at a particular event 'One dignified, elderly Tepoztecan rose and said, "Many people have come here to study us, but not one of them has helped us"' (1963: xv). Mindful of such expectations that research will produce tangible benefits, Gallaher was aware of the possibility that a researcher can, intentionally or otherwise, give 'the impression that he can determine the answers to all of a community's problems.... Some Plainvillers, at least for a time, viewed me as such a resource person, and there was the uncomfortable problem of having to convince them to the contrary' (1971: 296). The existence of numerous variations on these themes led Karen O'Reilly to conclude that researchers will search in vain for easy answers to the ethical issues that are raised when people invited to participate in research 'might give consent based on false hopes (that you might do some good for them or for the wider community)' (2012: 66). The purpose of research that does not have such benefits, of research that is purely 'academic', will not be immediately grasped by everyone. Community members may well reply to requests to participate in research with the question 'Is it worth it?' (Clark 2008: 958), especially if their previous experiences are of research that has not produced positive change. That said, Caroline Humphrey's experiences in Siberia provide some reassurance for researchers who feel that the generosity of people being researched far outweighs any benefits that they may receive. Suspected on her first visit in the 1960s of being a spy, over time she earned the farmers' trust and reports how she found it 'very moving to be surrounded with the warmth of people to whom one can give so little in return' (1998: xix). Her acknowledgement (1998: xviii) of local people's ability to do better than she had done in the first edition of her study (Humphrey 1983) says a good deal about effective ways of tapping into local knowledge.

Sources of fieldwork problems: Mutual misunderstanding about the purpose of research

The conclusion to which Vidich and Bensman were led about the controversy that followed the publication of their book was that it encapsulated the tension that exists for researchers between following scientific procedure and pursuing the maintenance of good community relations. Vidich wrote that 'the obligation to do scientific justice to one's findings quite often conflicts with the social obligation to please all objects of research'. His pejorative description of the practice of modifying information about people in order to secure their anonymity as 'fixing' (in Vidich and Bensman 2000: 429) data sets him apart from many other researchers, but his concerns about investigators on projects doing things in order to keep community members happy do have echoes elsewhere. Robert Moore, for example, prefaces his monograph on Peterhead with the warning that 'What I have to say in this book will not please everyone' (1982: x), mindful no doubt of the reception of his previous research (with John Rex) into race and housing in Birmingham which some community members felt had 'amplified their problems' (Rex and Moore 1981 :xvi). The commitment to portray something as it is has the potential to cause friction through the telling of uncomfortable truths. Srinivas and his colleagues refer to the tension inherent in the research situation in which the researcher is interested in the community members' 'dirty linen, which he cannot expect them to

wash before him' (2002: 5). Research extends to areas that people whose communities are being researched may prefer to keep private.

An important dimension of this issue relates to the status of knowledge about secrets and private matters. Many researchers have lamented their exclusion from communities that they were seeking to study on the grounds that they were suspected of being spies or government investigators (Crow 2008; Frankenberg 1990: 174; West 1945: ix), or at least of wanting to find out more than is appropriate about matters that are regarded as private by the people they are seeking to research. Frankenberg's sense of being on the outside is neatly captured in his account of how 'In my early days in the village I would often climb a hill and look sadly down upon the rows of houses on the housing estate and wonder what went on inside them' (Frankenberg 1969: 16), and it was only after he gained the trust of the villagers that he made headway with his research. Such success in turn generates other concerns amongst researchers. Pierre Bourdieu's *The Weight of the World* begins with his doubts about the consequences of being trusted: 'How can we not feel anxious about making *private* words *public*, revealing confidential statements made in the context of a relationship based on trust... True, everyone we talked to agreed to let us use their statements as we saw fit. But no contract carries as many unspoken conditions as one based on trust' (1999: 1, emphases in original). Different understandings about the purposes to which things said to a researcher will be put underlie many of the cases of dissatisfaction with what researchers do with community members' words.

Another aspect of mutual misunderstanding concerns fieldwork encounters in which the purpose of some research questions is not always apparent. Mike Savage and his colleagues have described how in their research conducted in Greater Manchester some of their white respondents struggled with being asked about whether they saw themselves as belonging to a particular ethnic group. Responses included 'A strange question that!... I'm not sure about the reasoning around that question' and 'What a bizarre question' (Savage et al 2005: 184), and sometimes, on the basis of how the interview had progressed up to that point leading to a hostile response being anticipated, the question was not asked at all. Similar experiences of questions being regarded as 'strange' have been reported by other community researchers (Neal and Walters 2006), and in the light of such experiences West's reputation among Plainville residents as an aggressive interviewer may be reconsidered. To pursue a question with research participants who do not see the point of asking it could be interpreted by interviewees as inappropriately pushy and intrusive at the same time as it is felt by researchers to be an important part of getting to the heart of the matter being investigated.

Conclusion

One of the purposes of reviewing the history of a field is that it confirms that many of the problems encountered by contemporary researchers are long-standing. This is demonstrated, for example, through Howard Becker's observation that 'Publication of field research findings often poses ethical problems. The social scientist learns things about the people he studies that may harm them, if made public, either in fact or in their belief' (1971: 267). The history of responses to such problems is also instructive. One piece of advice offered by Warwick and Littlejohn in the light of the reception of the Ashton study is that researchers should heed the 'warning to anyone who tries to comment without first reaching some agreement with the subjects of the research as to the appropriate

framework of analysis' (1992: 33). This advice does not use the language of the democratisation of research, but it is in the spirit of a trend in this direction that is embodied in the development of collaborative and participatory approaches to research. Reflecting on his own and other people's research in Asian villages, Otto van den Muijzenberg concluded that 'new insights in anthropology call for a high degree of involvement of those studied in the formulation of goals of research and a stronger representation of their, as opposed to the researcher's, interpretation' (1997: 344). This perspective involves a significant change in thinking, away from the idea that 'the anthropologist does fieldwork to advance his career and not to benefit those whom he studies', prompted in part at least by a recognition among researchers that the development of such instrumental relations with informants can leave the latter feeling 'sore' and used (Srinivas et al. 2002: 11). As Shah and Ramaswamy note, finding new and more effective ways of 'listening to the voices from below' (2002: vii) has been a particularly important innovation in how research in the field is conducted. This is not least because without this, if researched populations continue to feel badly treated, they may in increasing numbers decline invitations to participate in research in the future.

Among many examples of participatory work that could be cited in the field of community research, Eric Lassiter and his colleagues' (2004) collaborative ethnography *The Other Side of Middletown* is particularly appropriate to refer to here because it is a re-study for which the auspices were by no means favourable. The community being researched (Muncie, Indiana) had been extensively studied previously over the course of the best part of a century starting with Robert and Helen Lynd's (1929) original *Middletown*, but over that period the town's African American members remained more or less invisible in the many publications that researchers produced. In one way or another they had been edited out. The fact that in collaborative ethnography 'ethnographers work with local communities to construct their texts' (Lassiter et al 2004: 19) means that researchers cannot publish reports on their research that is at variance with community members' understandings in the way that much previous work in the field has been, and in this case it meant that Muncie's African Americans could not be overlooked. It also means that reports on how research has been received by community members can be more readily accepted in this type of publication than those claims made about their reception by authors of more conventional monographs.

The research relationships developed in collaborative approaches have the potential to reduce misunderstandings about the nature and purpose of research. John Dollard's comment that 'I expected to lose my friends in Southerntown when this book was published' rested on his belief that 'Candid analysis cannot be combined with friendship either in life or social studies' (1957: vii). Dollard's view was that the development of friendships was a necessary part of establishing the trust necessary to gain access to data that would otherwise remain hidden, and it is for these reasons that Srinivas describes 'winning the trust and friendship of the people being studied' as 'crucial' (1992: 144) (see also Arvidson, this volume). The cultivation of positive relationships among member members of the community being studied is a feature of many research reports (e.g. Breman 2007: xviii; Lewis 1976: 6). Gallaher, for example, mentions 'withdrawing from a large number of very close friends' (1971: 300) when he left Plainville, and refers to the tension between publication of outputs that contain academic assessments of community life and doing the right thing by friends in that community who made such outputs possible. In his case he did not suffer the accusations of betraying the people of the town that his predecessor West had, but the expectation of loyalty from friends is always going to be tested by publication of analyses of information gathered through

friendships. The statement of ethics produced as part of Lassiter and his colleagues' (2004: 20) research clarifies roles and responsibilities in the research process, and while such documents do not preclude friendships developing between researchers and members of communities being studied, they do make it clear that research relationships involve other, more formal elements than friendship.

Friendships with research participants are also mentioned by Scheper-Hughes, who became conscious of being regarded as a 'species of traitor and friend' by some of her fieldwork participants. Her reflections following her return to the community suggest that part of the problem may lie in the practice of anonymisation, since it 'makes us forget that we owe our anthropological subjects the same degree of courtesy, empathy, and friendship in writing that we generally extend to them face to face in the field, where they are not our subjects but our companions'. The 'brutally frank sketches of other people's lives as we see them' (2000: 325, 12-13) are not magically sanitised and made acceptable by anonymisation, because the issue at stake involves what is written as well as whether sources of information are identifiable. A further lesson of this and other studies is that there are varying levels of acceptability within communities. Publication of her book saw Scheper-Hughes lose some by no means all of her local friends; her return visit would not have been possible had that been the case. Warwick and Littlejohn (1992: 31) encountered not only critics of the original Ashton study but also people who defended it as an accurate portrayal of the town and its people. And alongside vociferous critics of West's book, Gallaher found other people whose view of it was that it 'was *largely* correct as far as it went' (1971: 292, emphasis in original). This should not surprise us. Community research tells us that communities are not homogeneous entities, but are characterised by diversity, including diverse opinions about the value of social research.

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